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WILLIAM HERSCHEL'S STAR SURVEYS.

"Cœlorum perrupit claustra."—*Herschel's Epitaph.*

Among the researches that I should wish to live to see undertaken by astronomers, and especially by the astronomers of America, who have shown so much originality in kindred inquiries, I regard with particular interest the survey of the stellar depths, in accordance with the original ideas of Sir William Herschel, but on principles such as he by no means supposed to be correct when he began his labors. It has been unfortunate for the work of research in this direction, that Herschel's ideas and results during forty years of observation have been dealt with, by astronomers who came after him, as though they had been presented in a single treatise, and indicated his views at some one given time. In a sense, there is something singularly appropriate to the grand subject with which he dealt, in this particular quality of the picture that we have received from his hands. The starlit heavens present a similar diversity in regard to time. We find it difficult, nay, impossible, to conceive that the stars as we see them are not as they actually are, nor even as they were at any given time. We do not see any star in its true place, even after correction has been made for such effects as are produced by atmospheric refraction and aberration of light. For each star is rushing swiftly through space, changing its apparent position in the celestial sphere, and although, owing to the enormous distance of each star, the apparent movement is not perceptible by ordinary eye-sight in less than hundreds of years, yet as light takes many years in reaching us from any star, it remains strictly true that the position apparently occupied by a star is not its real position, but one that it occupied long ago. Again, we do not see any star with its real light at the moment, but with that (by no means necessarily the same, even in amount) which it emitted many years ago. Even this is difficult to conceive; but this is little. Each star tells us of its history at a particular time, corresponding to its distance. Yonder bright star shows us its position and luster a score of years since; the less brilliant orb apparently close by, lies so much farther off that we must assign the news it brings us to at least a century ago. But many even of the brightest stars lie at such greater distances; while, when we pass to the fainter stars, we must often have to consider light-journeys of many hundreds or even many thousands of years. If we regard the telescopic view of the heavens as the real view presented to the eye,—at least, to the mind's eye of science,—we must recognize, in the case of the faintest stars seen by the most powerful telescopes, such vast distances that light cannot have come to us from those stars in less than hundreds of thousands, perhaps millions of years. So that the scientific view of the universe of stars has as wide a range in time as in space. We have no picture of the galaxy as it actually is, or even as it was, but of different parts inextricably intermingled, and at different, and very widely different, periods of time.

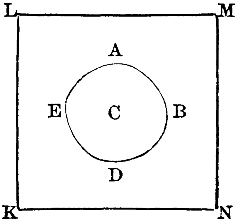
But science enables us to correct the mistaken idea that in the stellar heavens we see the universe of stars as it is at this very time. Though the mind may never be enabled to conceive the reality, and is, indeed, hopelessly unable even to approach the conception, yet the reason has been convinced long since that the stellar heavens tell the amazing story of vast realms of space and enormous durations of time, which modern astronomy has in part been able to read.

It is not very wonderful, but it is interesting and significant, that the labors of the man that has done most to bring the great problem of the star-depths before us should have been misinterpreted somewhat as we are so apt to misinterpret the heavens themselves. Writers even so able as Humboldt and Arago take statements from this and that part of Sir William Herschel's long series of papers, and set them side by side in the same page, or even in the same paragraph; nay, I have seen such statements wrought into a single sentence, when in reality they belong to entirely different parts of Herschel's process of inquiry, or even present entirely distinct views on the particular matter to which they relate. Although my chief work has long been to try to put myself in the position of those who are apt to make mistakes, in order that I may be the more successful in correcting such errors, I had not conceived it possible that so gross a mistake should have been made. Sir William Herschel suggested, in the course of his career as an observer of the stars, two entirely distinct methods of gauging the star-depths. They were so different in character, that — to take but one point of difference — one depended on the use of one and the same telescope throughout, while the other required that a series of telescopes of gradually increasing power should be employed. Yet, not only have superficial readers overlooked the characteristic difference between the two methods, and the reason why one method gave place to the other, but even those that have professedly undertaken the work of analyzing and abstracting the labors of the great astronomer of Slough, have fallen into the same preposterous mistake. I know of only one, Wilhelm Struve of Pulkova, who has clearly recognized and insisted upon the difference between the two systems of space-gauging that were employed by Sir William Herschel at the beginning and toward the close of his marvelous series of observations. Even Struve failed to recognize clearly that Herschel never did more than sketch in outline the results that would have followed from his second method of gauging, interpreted in a way that seemed to him likely to be sound and just. Herschel was too old to do more; and, apart from this, it may be said that he left those who came after him not only to apply the method fully, but even to interpret satisfactorily the few results that he had himself been able to collect.

Every one knows the nature of the system of star-gauging that Herschel at first adopted; in fact, it is the only one about which the great majority of students of astronomy know anything. It was the method suggested originally by Wright of Durham. Supposing all the stars visible in the telescope to belong to a certain system of stars tolerably uniform in size and distribution throughout (our sun being one of them), it is easily seen that if the telescope we use brings into view all parts, even the remotest, of this star-system, we can determine the shape of the system with considerable accuracy. For, in whatever direction we turn the telescope, we shall see a number of stars, greater or less, according as the boundary of the stellar system in that direction is farther or nearer. Wright of Durham applied this method of gauging, with a telescope of moderate power, with results closely resembling those that are presented to this day as among the chief triumphs of Sir William Herschel's entire series of labors. Wright found so many stars in the direction of the Milky Way, compared with the numbers seen in those parts of the sky that are free from milky light, that he was forced to assign a much greater extension to the stellar system in the direction of the Milky Way than elsewhere. Forced, at least, when we consider the assumption on which his inquiry had been based; for of course there were several other available explanations of the observed facts. Thus Wright was led to enunciate the theory, commonly attributed to Sir William Herschel, that the stellar system has the shape of a gigantic flat disk of stars, tolerably uniform in distribution. The Milky Way being divided into two streams along a part of its course as known to Wright, it was necessary to assume that the disk was cloven throughout half of its extent.

Sir William Herschel, making a more careful survey on the same plan, but with a much more powerful telescope, found that while in a sense this cloven flat disk theory was supported by the results he obtained, it was yet necessary to assign a much more complex figure to the stellar system, so long as the results of his gauges were interpreted in accordance with the assumptions suggested by Wright. It became clear that on these assumptions the bounding surfaces of the flat star-system were by no means smooth. Instead of a section of the stellar system through its center (near our sun) and at right angles to its median plane being bounded by straight lines, the outline must be of the most irregular form. Herschel drew one of these sections, which presented a shape somewhat like that of a long, dentate leaf. He appears not to have been at all struck by the peculiarities of outline thus presented, when he was considering only a section of the stellar system. It is obvious that a system of stars forming a sort of island universe might be expected to present many irregularities of shape, and a section athwart the middle of such a system might as probably be shaped like a toothed leaf as in any other way.

But as the work of survey went on, Herschel began to find that not only the particular cross-sections, but the system itself, presented peculiarities of form, and that these were related in too special a way to the position of the observer on the earth to be easily explicable as really belonging to the system of stars. Consider, for instance, such a case as the following: Over a certain region of the heavens, nearly circular, Herschel found that his star-gaugings invariably gave high numbers, while over the region all around this nearly circular space they as systematically gave very low numbers. Thus, if we suppose A, B, D, E to represent such a circular region, having its center at C, Herschel found that within the boundary A, B, D, E he always had fields of view rich in stars; while so soon as he directed the telescope to points outside of A, B, D, E, he found not more than perhaps four or five stars, instead of hundreds, in each field of view. The meaning of this result—if the assumptions adopted by Wright and Herschel are accepted—is obvious. Herschel himself never hesitated in recognizing this meaning; yet those who quote Herschel constantly, and regard with intense disfavor the idea that he could, under any circumstances, have made a mistake about the stellar universe, overlook the direct result of his observations, the result pointed out by himself and frankly accepted.



If within a small circular or roughly rounded space, such as A, B, D, E, many stars can be counted in every field of view, while over the whole space L, M, N, K, outside of A, B, D, E, few stars are seen, and if a great number of stars seen in any direction indicate a correspondingly great extension of the stellar system in that direction, then, of course, it follows inevitably that the stellar system extends toward the region A, B, D, E very much farther than toward any of the region around A, B, D, E. If the stars over the space A, B, D, E were uniformly distributed, the conclusion would be that a cylindrical projection or rod-shaped extension of the stellar system existed in the direction toward C, the center of this rounded, rich region of stars. If, on the other hand, as Herschel found to be almost invariably the case, the stars, though rich over the whole region

A, B, D, E, were much more closely aggregated near the center, C, than toward the edge, the conclusion would be that there was a conical projection of enormous length compared with its breadth, having its axis directed toward C. In neither case could the conclusion be regarded as reasonably likely, scarcely even within the bounds of probability. It would be strange enough to imagine a star-system of vast extent, with long cylindrical or conical projections extending from portions of the central group, the extensions being many times longer than the diameter of the parts of the central mass (the cloven flat disk of stars) from which they sprang. Nay, this would not only be strange, but altogether inadmissible when dynamical laws are taken into account. But if we overlook the strangeness and the unscientific nature of such a supposition, we find another and overwhelming difficulty in the peculiarity that every one of these strangely projecting cylinders and cones of stars must be conceived as having its axis directed exactly toward the solar system, from a member of which we make our observations. Our sun is, by the very assumption on which the system of numerical star-gauging depends, but one among millions of suns forming a system of stars. There is no reason whatever for supposing that he lies at the center of the system, or, indeed, that the system is of such form as to have a "center of figure," which, of course, can only exist in the case of a symmetrical system. On the contrary, there are abundant reasons in the complex form and various degrees of brightness of the Milky Way, and in the general superiority of luster found within its southern portions, for believing that the system of stars (if, indeed, the Milky Way represents its richer parts) is exceedingly complex in shape, and the sun eccentrically placed within its limits. Yet, as seen from this casual star—for in looking from the earth we get, to all intents and purposes, the same view of the stellar depths as if we looked from the sun — all the strange projecting spikes of stars (I can think of no more suitable name) are foreshortened into the appearance of round star-clusters! This is absolutely incredible.

There can be no doubt or question as to the significance of the observed facts, if the assumption on which the star-counting method depended is accepted, and it is scarcely possible to entertain any doubt or question as to the absolute inadmissibility of the result thus obtained. If the greater the number of stars seen in any field of view, the greater is the extension of the star-system in the direction of those stars, there must be enormous spike-shaped projections of stars wherever clustering aggregations are seen, along the Milky Way or elsewhere; while the existence of such projections, always directed exactly toward the sun, cannot be admitted as possible by any reasoning mind.

Sir William Herschel, at any rate, felt no doubt on the subject. He saw at once, that since the principle he had assumed in the beginning of his star-gauging by the counting method led to a result that was manifestly preposterous, the principle that had seemed so reasonable must be rejected as unsound. Repeatedly we find him saying that a long-continued examination of the star-system has convinced him that the idea of uniformity of distribution, which he had imagined at the beginning, must be given up as inadmissible. He remarks that he has satisfied himself that the stars in the Milky Way are distributed very differently from those in our neighborhood. He understood the real meaning of the clustering aggregations of the stars along the Milky Way, regarding these as manifestly real clusters of stars, not stellar projections.

It would indeed matter little if Herschel had failed to recognize the meaning of what he had himself observed. Had he so failed, we should have found but another instance among hundreds known to us of the inaptitude of even the keenest observers to analyze their observations, and educe the full meaning of what they have discovered. Herschel differed from the rank and file of mere observers—the writing army of science—in the power he possessed in this respect, until approaching the end of his wonderful observing career. But had he in this case failed to reason right—as in later years we find he actually failed—this should in no sense influence our judgment respecting facts that are as clearly before us as they were before him. We know that the assumption he first adopted would compel us to assign to the star-system a shape that is antecedently unlikely even as a shape, and is rendered utterly inconceivable when we take into account the peculiar relation of all its most marked features to the sun. If we saw a number of grains scattered over a surface at random, and found that as they fell they arranged themselves in the form of a star, all the radiations of the star-form being directed exactly toward a certain mark on the surface, we should be absolutely certain that there were peculiarities in the surface, differences of level, or the like, which brought about this result. It could not possibly be accidental. We should feel as certain that there cannot be multitudinous radiating streams of stars, all extending straight from our sun, unless there is some special peculiarity in our sun to cause this singular conformation of the star-system. And since we know certainly that no such peculiarity exists, we cannot but reject decisively the belief that the star-system is so shaped. It could make no difference whatever in our conclusion that Sir William Herschel had failed to notice the inference directly deducible from his observations. But, as a matter of fact, the elder Herschel accepted the rich clustering regions along the Milky Way as in reality what they appeared to be, that is, as clusters, not as projecting streams of uniformly strewn stars.

Of course, the principle that he had assumed as the basis of this system of star-gauging—the principle of generally uniform distribution—had to be abandoned in at least these special cases. Probably Herschel was not prepared to admit that it must be given up altogether. This seems much clearer in our time, with our vastly increased knowledge about the stars, than it could have been to Herschel, keen though his insight into such matters unquestionably was.

But Herschel went on at this time with a series of sidereal observations of the widest scope and the most diverse character. He had practically the whole field of stellar and nebular research; the universe was all before him where to choose, a noble but truly a bewildering scene. So far as observational work was concerned, he could hardly go wrong, let him undertake what portion of the survey he might. Again and again he sent to the Royal Society the results of fresh series of observations —now a thousand or so of new nebulæ discovered by him in his "sweeps" of the mighty dome of the heavens; anon the survey of regions containing hundreds of thousands of stars; then an inquiry into the distribution of nebulæ and stars; and all this work went on in company with the observation of sun, moon, planets, and comets, the construction of new telescopes by hundreds, the study of many complex physical problems, and other scientific inquiries of minor importance.

As these labors went on, and clearer ideas of the constitution of the heavens presented themselves, Herschel must have begun to see that the system of gauging the galaxy by counting stars was utterly inadequate. With all the various orders of star-clusters and nebulous masses, how could he longer imagine that mere numerical wealth of stars, or of points of light looking like stars, indicated enormous extension in the direction of the line of sight toward such regions? Distance, indeed, he felt to be indicated by the close aggregation of multitudinous points of light. But the vast distance that he recognized in some of these clusters of stars was something entirely different from the long array of stars in particular directions that he had originally assumed as the explanation of great wealth of stars in such directions. His original idea of the structure of the stellar universe had not included the conception of star-clusters, either of the larger sort, such as he had found in parts of the Milky Way, or of the smaller kind, rounded, elliptical, irregular, ring-shaped, and other forms of small clusters, which sometimes he was disposed to regard as external stellar universes, at others as fragmentary portions of our own galaxy.

It was a natural outcome of such observations as these, and of the doubts they inevitably cast on Herschel's original method of star-gauging (or rather of the conviction forced upon him that the principle of that method was untrustworthy), that Herschel should be led to devise another method. I wish specially to show that the method he now adopted was entirely different from the other, insomuch that it is among the marvels of misapprehension that the study of science brings before us that this method should be confounded with the earlier system. But I wish also to show how naturally the new method of star-gauging arose out of the observations on which Sir William Herschel had been engaged since his earlier star-gauging had shown him that the universe of stars is not constituted as at first he supposed it to be.

Let us take the latter point first. Among the nebulæ Herschel had found all orders of what he called "resolvability." Some of them are clusters so coarse in texture that it was not easy to draw a line of distinction between them and the more clustering portions of the galaxy itself. I may notice in passing a feature that was not known to him, viz., that the nebulæ of this coarsely clustering type are more numerous upon and in the neighborhood of the Milky Way than over the rest of the heavens. Others, again, are compact clusters, still easily resolved into stars with a telescope of moderate power. Then there are others so difficult to be resolved into stars, that until powerful telescopes were applied they presented the appearance of round or elliptical cloud-like spots. Yet others are still finer in their starry texture, so that only a few of the most powerful telescopes in the world will resolve them into discrete points of light. And lastly, so far as the nebulæ of regular shape are concerned, there are some that have not yet been resolved into stars by any telescope. It is noteworthy that, arranging the nebulæ into classes in the order of their resolvability, those most easily separated into stars show the most marked tendency to aggregation along the Milky Way, and are irregular in shape. Those that come next in order are nearly circular, and though still showing a certain increase of wealth toward the Milky Way, are found in tolerable frequency elsewhere over the star-sphere. The nebulæ that are resolvable with difficulty, on the other hand, are elliptical, and are absent altogether from the Milky Way. These points are manifestly associated with the great problem of the constitution of our galaxy, though not directly related to Sir William Herschel's observations. In fact, though he noticed the remarkable circumstance that the nebulæ cluster near the northern pole of the Milky Way (that is, near the point farthest on the northern heaven from the central line of the Milky Way), he did not recognize the manner in which this peculiarity is associated with the character of the nebulæ, and he supposed that the nebulæ are rich along a ring-shaped region akin to the Milky Way, but at right angles to it, and formed of star-clouds instead of stars.

Recognizing these diversities in the structure of nebulæ, Herschel was naturally led to regard them as due to differences of distance. He supposed the coarser clusters to be the nearer, and the finer in stellar texture to be the more remote. All nebulæ might fairly be regarded, at that stage of the inquiry, as farther away than the stars forming our own sidereal system, even to the farthermost parts of the galaxy. Herschel does indeed speak of the possibility that toward the side of our flat sidereal system, as he viewed it, there might be room for the nearer approach of the parts of a former single great nebula, as though the nebulæ seen clustering in great numbers over the wings, shoulders, and head of Virgo might be but the parts of a former nebula of gigantic proportions. But this notion seems not to have been more than a passing idea with him, or to have much influenced the development of his views. The idea gradually gained force, on the contrary, that in the greater or less telescopic power needed to resolve a nebula, or a group of stars, we may find evidence of the greater or less distance of the object so scrutinized. So soon as this idea had taken firm root in his mind, which was not till toward the end of his observing career, he proceeded to put to use this means (as he supposed) of determining distance. I refrain from saying that he put it to the test, for I have no evidence that he consciously did so. He seems to have taken it for granted that the visibility of a star as a separate point of light, by a telescope of given power, was in itself a test of distance. He stated the principle, and showed how it might be applied to stars, star-groups, star-clusters, and nebulæ of various orders; then he proceeded to employ it as a means, first, of measuring the scale on which the stellar system is constructed, then of determining its shape, and lastly, of ascertaining the distances of the nebulæ.

And now to show how entirely distinct was this method of gauging the star-depths from that which Sir William Herschel had before employed. We may call the first method star-gauging by enumeration; the second, star-gauging by resolution. In the first method, the same telescope (a powerful one) was to be applied to different parts of the star-depths, the number of stars counted, and, as the number was greater or less, the limits of the stellar system in the given direction were assumed to be farther away or nearer. What was taken for granted in this method was, first, that the stellar system is formed of stars generally uniform in distribution throughout the system; secondly, that the telescope employed was powerful enough (it was eighteen inches in diameter) to reach to the limits of the system; thirdly, that there are no vacant spaces in the system.

In the second method, different telescopes, ranging in power from the weakest in use to the most powerful he could make, were directed to each region examined, until the whole region had, if possible, been resolved into stars well defined on a black background, without any trace of milky nebulosity. What was assumed in this method was, first, that the sidereal system is formed of stars not differing greatly from one another in size; secondly, that in the various clustering regions throughout the sidereal system the average distances between stars are tolerably uniform, or, in other words, that what may be called the stellar texture of each part of the system is the same throughout, though there may be vacant spaces in some parts, and clustering aggregations of various forms in others; thirdly, that any part of the system that the most powerful telescope he employed failed to resolve, lay at a distance beyond the gauging or fathoming range of that telescope. To bring the two methods more clearly into contrast, note that

In Herschel's first method of gauging, it was essential that one and the same telescope should be used throughout the work.	In Herschel's second method of gauging, it was essential that a series of telescopes differing in power should be employed.
The comparisons made related to different fields of view, seen with the same light-gathering powers.	The comparisons made related to the same field of view, seen with different light-gathering powers.
The inference deduced related to the extension along the line of sight of the objects connected with the one telescope employed.	The inference deduced related to the distance of objects seen with the different telescopes employed.

Had Herschel been a younger man when he thought of the second method of gauging the star-depths, it is probable he would have felt from the beginning that the method was one to be tested before it could be trusted. He would have been prepared to find that while, if his assumptions were sound, his results would have such and such a meaning, it was at least possible that his results might show that his assumptions were altogether inadmissible, and therefore that his new method of star-gauging was altogether unsound. But Herschel was nearly seventy-nine years old when he began to employ his second system of star-gauging, and though he still possessed much of his skill as an observer, he had lost much of that versatility of mind which had enabled him not only to observe skillfully, but so to analyze his results as to see whether they were consistent with the assumptions by which they were to be interpreted. Can we wonder if at that advanced age Herschel was content to work resolutely at the task on which he had entered, without considering very closely or thoughtfully the question whether the principle by which he proposed to interpret his results was sound or otherwise? It had seemed to him so reasonable as to appear almost unquestionable; we do not find a line or a word tending to show that he ever questioned it. The principles on which the first method of star-gauging had been based had seemed to him equally unquestionable at first; but he had found them to be unsound by noting that his observations interpreted by means of them led to absurdities. The observations made in accordance with the second method of gauging led in like manner, if interpreted by means of the principles on which that system was based, to absurdities. But this, his attention being directed too exclusively to the results themselves, he failed to recognize.

Herschel began this new work of star-gauging by examining individual stars. It is clear that the principle of the method is applicable to a star as readily as to a star-cluster. If we can determine the average distance of those stars that we can just see with the naked eye on a dark and clear night, and stars generally throughout the stellar system have the same mean size (by which I mean that the average for a thousand stars in any one part of the system is the same as for a thousand stars in any other part of the system), then, of course, a telescope increasing the light-gathering power of the eye fourfold will just show a star twice as far away; one increasing that power ninefold will just show a star three times as far away, and so forth. It was by observations made in this way that Herschel was led to the belief that among the stars shown by his most powerful telescopes are some that are thousands of years' light-journey from the earth. Singularly enough, the very evidence that shows in this case that the principle of the new method of star-gauging failed, has shown that the same result can be inferred that Herschel based on that principle. We know now, for example, that many of the brightest stars—as Sirius, Capella, Vega, Arcturus, and Aldebaran—are much farther away than some—as 61 Cygni—that are barely visible to the naked eye on the darkest and clearest night, instead of these being (as they should, if the principle of the new method were sound) fully a hundred times farther off. We cannot, then, any longer assume, as Herschel did, that the faintest stars seen with his largest telescopes, are thousands of times farther away than those forming our constellations. They may be relatively near, and look small because they really are much smaller than their fellows. But while, on the one hand, we cannot now suppose faint stars to be necessarily far away, we are precluded, on the other hand, from inferring that bright stars are necessarily near. Since it is certain that many of the brightest among the stars visible to the naked eye are really farther away than many of those that are barely discerned, we may infer, with considerable confidence, that the same holds in the case of the field of view of the mightiest telescope yet made. Now, the faintest stars seen in such a field are those that would be the brightest in fields of view obtained by penetrating still more deeply into space. Among them, therefore, must be some farther away than those yet fainter stars; among them, in fact, are probably stars like Sirius, Canopus, and Alpha Centauri, which owe their brightness to real vastness, and lie at depths remoter than the daring conception even of the elder Herschel has suggested.

But it is when we turn to the study of star-clusters that we recognize at once how thoroughly the principle of the new method of star-gauging was disproved, and how important, nevertheless, are the results that Herschel's observations on the new plan established. If he had found that each cluster, whether in the Milky Way or of the nature of a star-cloud, had been resolved by the application of a certain telescopic power, or of powers ranging between tolerably close limits, he might logically have been content to believe that his principle was sound. An easily resolved cluster would be set relatively near, and one resolved with difficulty would be set far away. But as a matter of fact, he met with a very different result in many cases; and a single case of the kind would have sufficed to dispose of the principle he had adopted. He found clustering regions (rounded in form) that were partly resolved by even his weakest telescopes, and more and more resolved on each increase of telescopic power, until he brought into action his very largest telescope; but even with this instrument, milky nebulosity still remained. This peculiarity would be limited to a certain rounded space, in some cases not so large as the disk of the full moon.* Nichol says of these regions, "What wonder if even Herschel shrank back appalled in the presence of these unfathomable abysms?" Herschel himself spoke less turgidly. He simply says, "When I have been unable to resolve the Milky

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* Herschel himself does not dwell on this particular point, though it could not possibly have escaped his attention; but any telescopist can ascertain for himself that all round the "unfathomable" regions noted by Herschel are regions that even moderate telescopic power will completely resolve.

Way with my most powerful telescopes, it has been because the Milky Way is unfathomable." *

Now, this observation, interpreted by the principle of the second method of star-gauging, leads to precisely the same absurdity to which Herschel had been led by his first method, and still more definitely, though not quite so obviously. All round one of these regions that he found unfathomable, the star-depths were easily fathomed, and therefore in those directions the stellar system had no great extension. But in the direction of these unfathomable regions the star-system had an enormous extension, if the principle of the new method could be trusted. The case is precisely the same as though a surveyor of the depths of ocean found that all over a large area of the sea bottom, save one spot, a few yards perhaps in length and breadth, he reached bottom with a hundred fathoms or so, while at that spot he could not reach bottom with a line of two or three thousand fathoms; except that, marvelous as such a deep and narrow hole reaching straight down two or three miles, but only a few yards across, would seem to the observer taking such soundings, it would be easy to explain, compared with the sidereal phenomenon that Herschel had before him. We can imagine causes for a deep vertical hole in the earth's crust, but we can neither imagine any cause for a straight star-chasm projection of the galaxy in a direction exactly from the sun, nor admit the possibility that such a projection could continue if it had ever existed. That there should be several such projections would be simply impossible, even if we admitted the possibility of the existence of one.

But this result, which thus conclusively proved that the principle of the new method of star-gauging was unsound, established nevertheless a most interesting fact. Since the clustering regions that yielded in part to Herschel's weakest telescopes, but not wholly even to his most powerful instruments, could not

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* Struve was led into a singular mistake by this sentence (which I quote from memory, but correctly in essentials). He wrote it out probably in German, "Wenn Ich," etc.; or if not, he simply understood it as if the English word "when" were equivalent to the German "wenn"; for in his "Études d'Astronomie Stellaire" he writes the sentence with the word "Si" for "when," making the statement, which Herschel applied to those parts only of the Milky Way that he could not fathom, relate apparently to the whole of the Milky Way, and suggesting consequently an infinitely extending flat, galactic disk for Herschel's finite one.

possibly be long, straight, projections similarly constituted throughout their length, it follows that they must be clustering aggregations presenting a wide variety of stellar texture. There must be larger stars separated by wide intervals, stars not so large and separated by intervals not so wide, and stars smaller and smaller in real size and set more and more closely, till even with Herschel's most powerful telescope, they could not be separately discerned. In other words, instead of penetrating more and more deeply into space, as he supposed, he was in reality scrutinizing more and more closely the stellar structure of one and the same region of space.

This variety of feature within clustering regions of the Milky Way would have appeared strange to Herschel (in fact, the idea scarcely presented itself to him), but in our time it appears the most natural thing in the world. The analogy of the solar system, as known to us, suggests precisely such variety of structure in the greater system that Herschel was studying. Analyzed by optical powers varying in range from unaided vision to the keenest telescopic scrutiny yet available, the solar system presents a constant increase of complexity. The eyes see sun, moon, and a few planets; the telescope reveals more planets, some really as large as Uranus and Neptune, but faint through vastness of distance; others nearer than Saturn and Jupiter, but looking faint because small; and yet others associated with the larger planets as dependent orbs; more and more bodies come into view with closer and closer scrutiny of the solar domain; yet portions still remain unresolved, such as the Zodiacal region, where astronomers more than suspect that millions of millions of nerolites and meteorites are traveling around the central orb. With this knowledge for our guidance, it seems as strange to the thoughtful student of the heavens in our time to regard the stellar system as generally uniform throughout in texture, as the diversity of texture that we recognize in the solar system would have appeared to Herschel.

Observations of star-clouds regarded by Herschel as external galaxies, should have led him (and doubtless would in earlier years) to a similar conclusion. It is true that in many of these systems there is an apparent uniformity of stellar texture consistent with the idea that they are formed of stars of about the same size, and strewn with general uniformity through the whole region occupied by the star-cloud. Most probably, indeed, the consideration of these features encouraged Herschel in the belief that our own galaxy is similarly uniform in texture. Moreover, in comparing one star-cloud with another, Herschel was not necessarily led to recognize the possibility that, even as one star differs from another in glory, so the nebulæ may differ much from one another in structure, regarding them for a moment as he did, that is, as external galaxies. But there was a simple yet absolutely fatal objection, in the results that he obtained, to the theory that ran through all his work at this time, viz., that not only is the texture of our own galaxy uniform throughout the extent of the stellar system, but the same sort of star-texture exists, with considerable general uniformity, among all the island universes within our ken. Herbert Spencer was the first to note this objection; but it occurred independently to me (it is, indeed, obvious) in 1867, when I had not as yet read a line of his works. That it did not occur to Herschel himself, shows clearly how unready, in his extreme old age, he had become to analyze his results as he had in earlier years. Herschel had found parts of our galaxy unfathomable, which showed that, in accordance with his assumptions, the outermost extensions of the galaxy are beyond the resolving power of his mightiest telescope. But the nebulæ, if they are external galaxies, must lie hundreds of times farther away than the outermost parts of our own galaxy. For each one of them, from its observed size, is known to lie at a distance exceeding hundreds of times its own diameter—that is, the diameter of our galaxy, on the assumption that galaxies are all of about the same size. Thus, then, we have this absurd result, that, whereas parts of our uniformly textured galaxy, at a distance of half its diameter, are irresolvable by the most powerful of Herschel's telescopes, many similar galaxies, hundreds of times farther away—corresponding to the diminution of their light tens of thousands of times—are resolvable with telescopes of much smaller power! Manifestly the principle of the second gauging method fails here again for the third time, and most hopelessly. Whether the star-clouds are external galaxies or not, the principle that Herschel had adopted for their interpretation, and in order to bring them into comparison with our own stellar system, must be given up.

But we know now—I venture to speak of it as certain, though many suppose it to be but a theory of my own—that the nebulæ are part and parcel of our own galaxy. Herschel's results went far to prove this, and had he but analyzed them he would have seen as much. Not only does our galaxy differ greatly in texture in its various parts, but it is as varied even in constitution as our solar system, or, rather, it is doubtless infinitely more varied in reality, but presents obviously to us the evidence of only about the same degree of variety. As in the solar system there are large planets and small ones, so in the stellar system there are stars of many orders of real size; as in the former we have streams of tiny bodies, like the asteroids, so in the galaxy we find streams of small stars, as in the Milky Way; as in the solar domain there are meteor-clouds and comets partly or wholly gaseous in structure, so in the great galaxy to which our sun belongs there are clouds of star-dust and mighty masses of nebulous matter (chiefly gaseous), like the Orion nebula.

I may hereafter give a brief sketch here of the evidence respecting the architecture of the stellar heavens already obtained by astronomers. In such a sketch the work of the Herschels would hold a prominent place. I may also show the methods of survey that commend themselves for future employment. My present object has been, first, to show how entirely distinct were the two methods of star-gauging that many who suppose they know something of Herschel's work have hopelessly confounded together; secondly, to point out how thoroughly the application of each disproved the assumptions on which either had been based; and lastly to show how, nevertheless, the results obtained by each method threw useful light on the great problem that Sir William Herschel, first of all men, successfully attacked by observational methods.

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